NAVY DEPARTMENT

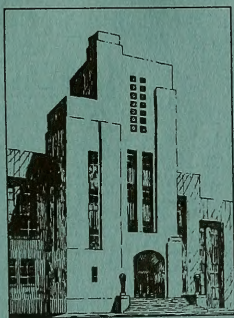
THE DAVID W. TAYLOR MODEL BASIN

WASHINGTON 7, D.C.

DISCUSSIONS AND AUTHOR'S CLOSURE FOR A BRIEF SURVEY OF PROGRESS ON THE MECHANICS OF CAVITATION

by

Phillip Eisenberg



June 1953

Report 842A

NS 715-102

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Presented to the Chesapeake Section
Society of Naval Architects and Marine Engineers
18 October 1952

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DISCUSSION

Dr. F. H. Todd, Chief Naval Architect, David Taylor Model Basin

Cavitation is a phenomenon of great importance to the naval architect and it may be of interest to summarize some of the effects it may have on ships, their propellers, and appendages. The first, and most serious of these, occurs in cases of well-developed propeller cavitation in which there may be a considerable loss of thrust and therefore of speed. This was first noticed in the trials of the British destroyer "Daring" about 1892. Cavitation on such propellers also gives rise to hull vibration and to damage to the blades of the propellers. This latter takes the form of an erosion which may develop very rapidly and make frequent renewals of propellers essential. Cases are on record where trans-Atlantic liners have had to have new propellers after no more than two round trips across the Atlantic and of destroyers which have shown the beginning of propeller blade erosion after only two hours at full power. Concurrently with vibration and erosion, cavitation also gives rise to undesirable hydrodynamic noise. The lifting surfaces on hydrofoil boats also suffer from cavitation as the foils approach the surface and thus may give rise to instability in the motion of the craft. It may be said therefore that cavitation is an unmitigated nuisance to the ship and propeller designer.

In the past we have generally been able to reduce the effects of cavitation by good design practices, such as larger propeller blade area, the use of constant pressure sections, and careful attention to the shape of the leading edge of the blades, but we are now entering the region where these solutions are becoming less useful. Thus, in destroyers and carriers, we must already accept some loss of speed at the top powers, while in high-speed motorboats the propellers are actually working in the fully-cavitating or super-cavitating region; that is, where the whole of the back of the blades is denuded of water with a consequent great decrease in efficiency. This latter may be as low as 30-35%, although it does result in relative immunity from erosion of the blades. Cavitation from propeller blades or

hull appendages is also the major source of ship noise, and the delay in the inception of the former, by even a few knots, is of extreme value for this reason alone. Hydrofoil boats of small size offer attractive possibilities at high speed together with the ability to ride over relatively severe waves. Here again cavitation is one of the limiting factors hindering further development.

It is obvious that new ideas and new methods must be enlisted if we are to make further progress, and Mr. Eisenberg has shown us today how he and his branch of the Hydromechanics Laboratory are exploring the fundamentals of the subject. Only in this way will we be able to proceed with confidence to the attack on these new problems and the development of rational design criteria for the use of the naval architect and marine engineer.

Mr. Eisenberg has more than once mentioned in his paper the effect of air content of the water upon the incidence of cavitation. The program of experiments on propeller cavitation, carried out under the auspices of the I.T.T.C. in many propeller tunnels throughout the world, has shown the critical importance of this factor and the need for its control in all experimental work. I was interested in Mr. Eisenberg's statement that, in his opinion, only the entrained bubbles and not the dissolved air contributed to the inception of cavitation. This is a matter of some importance to propeller tunnel work, for here we measure the total air content. We are fully aware of the urgency of the problem, as may be judged by the fact that in the new 36" propeller water tunnel we are to have a resorber necessitating a pit 80' deep into the bedrock on which this station is built.

I would like to know if the author has any views on the correct way to scale cavitation phenomenon as they affect propellers. At present we find the best correlation between model and ship is found when we run the propeller in the tunnel at the highest possible water speed, ignoring all Froude scaling, and adjusting the revolutions per minute to give the correct slip. This gives relatively small pressure reductions on the water, with the result that the water retains the greater part of its air content. In connection with this scaling problem, it may also be of interest to state that the geometrically similar propellers used in the international comparison, which range from 8 to 18 inches in diameter, give considerable differences in performance.

It is generally believed that the erosion damage on propeller blades and on rudders and other fittings abaft the propellers is due to the collapse of cavitation bubbles

on the surface of the material. As long ago as 1890, Sir Charles Parsons carried out calculations on the probable pressures exerted by the collapse of such bubbles, while today Dr. Harrison has given some figures suggesting that such pressures can reach extremely high values. It would be interesting to have Mr. Eisenberg's views on this aspect of the cavitation damage problem.

Dr. H. W. Lerbs, David Taylor Model Basin

The two papers by Mr. Eisenberg on the Mechanics of Cavitation represent a comprehensive review and excellent discussion of the available literature on this subject. There are, in my opinion, only a few points which may be worth while to be added to Mr. Eisenberg's representation which points are related to the special case of cavitation on a propeller.

Firstly, I want to make one remark relative to the laws of similitude for propeller model tests. These tests are usually conducted, as mentioned by Mr. Eisenberg, with the cavitation number, referred to the center of axis, as the predominant parameter and with the Reynolds' number chosen greater than a certain critical number. Doing so, an unsimilarity takes place due to the fact that an element of the propeller rotates in a vertical plane. This implies that the static pressure at an element is a periodic function of position which requires a suitably defined Froude number to be satisfied for the model test. However, this is usually in contradiction with an overcritical Reynolds' number and, hence, the Froude number is disregarded. Considering then the cavitation number of a propeller element, its average equals that of the full scale propeller but both the amplitudes and the periods are different. The error which arises depends on how the cavitation performance of a section depends on a periodically changing cavitation number for an equal average of this number. I have formerly tried to answer this question by experiments with a series of geometrically similar model propellers varying only one of the significant parameters and keeping the others constant. Except for a small variation of the Weber number, this could be achieved when working with different combinations of water temperature and absolute size of the models. These experiments showed that the force coefficients of a propeller model depend on Froude's number, i.e., on the amplitudes of the local cavitation number. However, to obtain a clear answer to the effect of a periodically varying cavitation index, such tests should be carried out with a hydrofoil and not with an assembly of different hydrofoils as represented by a propeller.

With respect to the inception of cavitation, no clear and definite effect could be found from these tests beyond a critical Reynolds number which is in contradiction to the measurements by R. W. Kermeen (Fig. 2 of part II) from which the critical cavitation number not only depends on absolute size but also increases when the Reynolds' number increases within the whole range investigated.

For tests on similitude of cavitation the Weber number is one of the entering parameters, the determination of which requires the surface tension to be known. This quantity depends on the composition of the content of the cavity which is not reliably known. The method which I have tried to use for determining the surface tension in the afore mentioned tests is related to tip vortex cavitation. Ackeret has shown that a sinusoidal shaped surface of a cavitating vortex core is in equilibrium under the action of centrifugal forces and of surface tension and has established a relation between wave length, wave height of the deformations and the circulation of the vortex. Taking a hydrofoil at a certain angle of incidence, the lift coefficient and, from this, the circulation of the tip vortex are known. From photographs of the cavitating tip vortex, wave length and wave height of the surface are determinable from which, together with the circulation, the surface tension of the tunnel water against the gas content of the cavity follow.

I merely want to mention in this connection, that apart from the sinusoidal shaped deformation of the surface of a cavitating vortex core (which does not depend on time) progressive waves are possible on the surface under the action of the centrifugal force field, both in the axial and in the circumferential direction, which may have some relation to the excitation of singing of propellers. I am sure Mr. Eisenberg will include these phenomena on the free surface of cavitating vortex cores in the final edition of his report.

My last point refers to the forces on cavitating bodies. In order to determine the lower limits of thrust coefficient and efficiency of a propeller we are much interested to know how lift and drag of a section behave when the cavitation number decreases to zero. There are some experimental results on sections with fully developed cavitation available which have been carried out by Walchner and theoretical considerations have been made by Betz. However, the tests are conducted at small Reynolds numbers and the approximations by Betz which is based on the Kirchhoff flow is restricted to the case where the free stream line starts from the leading edge and does not include the practically important case where the free stream line starts at the point of maximum thickness of the section. Such work on bodies with fully developed cavitation which produce lift would be highly desirable.

Mr. Philip Mandel, U. S. Naval Bureau of Ships, Washington, D. C.

The author emphasizes again in this excellent status report, the intimate association between the inception of cavitation and the presence of nuclei of foreign material in the liquid. There is an old naval architect's problem that has always puzzled me and I wonder now if the above is not at least a partial answer to it. The problem is that of the severe scale effect demonstrated many times in the past in the phenomenon of rudder breakdown. The evidence is that model rudders break down even within the speed-range of models which, of course, is very low, being based on the Froude Scaling Law. On the other hand the full scale rudder, rarely, if ever breaks down within the speed range of the ship. Back in 1945 in a discussion of this problem the Model Basin intimated that this pronounced scale effect might be due to the fact that the pressures do not scale properly between model and full scale. Carried to its logical conclusion that explanation would lead me to believe that the ship rudders should break down sooner than the model rudders! A somewhat more plausible explanation has been offered in recent years. It is claimed that the model rudder is very likely in laminar flow because of its very low Reynold's Number. That flow it is thought, breaks down into separation and cavitation more easily than does the turbulent flow about a full scale rudder. Hence the failure of breakdown to scale properly.

So much for previous explanations of the problem. The author has mentioned the existence of cavitation within turbulent flows and particularly within separated flows; it therefore probably exists within the flow about a rudder after breakdown. Since sea water must have vastly different foreign material content than Model Basin water, perhaps the explanation of the described scale effect lies in the different liquids rather than different flows. The Author's views on this line of thought would be appreciated.

Professor M. S. Plesset, California Institute of Technology

This paper by Mr. Eisenberg maintains the high standard of his report of 1950 "On the Mechanism and Prevention of Cavitation". Eisenberg's survey of the developments in this field since 1950 shows the increased effort on this aspect of hydrodynamics. In spite of the increased effort, one definitely has the impression that new problems are exposed at least as rapidly as the old ones are solved.

I should like to make a comment on Eisenberg's discussion of the effects of gas content and nuclei content on the inception of cavitation. We have recently observed, at low dissolved

air content, a "hysteresis" effect in the inception of cavitation. As the static pressure is reduced in the working section of the High Speed Water Tunnel at C.I.T., one can go to pressures below those at which cavitation usually begins. This state of tension in the flow is unstable and cavitation can be initiated by a small disturbance in water tunnel conditions. The same phenomenon has been observed by Parkin in the Garfield Thomas Tunnel at Penn State. Other workers have of course been aware of the fact that tensions can be produced in ordinary water, but it is of interest to note that the effect can be found in the gross conditions obtaining in a water tunnel.

We at the California Institute of Technology are particularly interested in the observations both here and at the Taylor Model Basin on the nonappearance of rebound in the collapse of cavitation bubbles. One might expect that the reversal of the liquid motion following collapse would lead to such tensions in the water that a cavity would reopen. Our observations at low air content are, however, unambiguous in showing no rebound. We have here very possibly another example of ordinary water withstanding tensions for a short-period of time. As compared with the observations mentioned above on the state of tension in a water tunnel flow, one would expect that the tensions following a vapor bubble collapse are much higher but the duration of the tension is no doubt much shorter.

Eisenberg also reviews the theoretical results of M. Rattray on the collapse of a cavitation bubble near a solid boundary. Rattray showed that the effect of the boundary was to introduce large distortions in a bubble which begins its collapse in a spherical shape. He also found that the collapse time was lengthened. I should like to point out here the possibility of the appearance of distortions in an initially spherical bubble which collapses in an infinite liquid. These distortions would appear now, not as a consequence of an adjacent boundary, but because the collapse motion of a spherical cavity in a liquid is unstable. These deviations from the simple spherical shape would be of importance for the further development of the theory of cavitation bubbles.

In concluding my discussion, I should like to say that the relatively small group of workers in this field are grateful to Mr. Eisenberg for his excellent review of the progress in the mechanics of cavitation. He contributes to this field not only by his own work at the Taylor Model Basin, but by his critical examination in his review reports of all the work being carried on in this complex and difficult field.

Dr. Paul Dergarabedian (Communicated by Mr. G. V. Schliestett), Naval Ordnance Test Station, Pasadena, California

The inception of cavitation in terms of the dynamical behavior of a vapor or gas bubble involves the consideration of the growth of such bubbles from an initial equilibrium size. This problem has been considered in the Cal. Inst. of Tech. Hydro. Lab. Report No. 21-10, August 1952. In this report calculations are presented for the dynamic stability of vapor and air bubbles in superheated water. These calculations indicate that the values of the bubble radii for which the equilibrium is unstable are restricted to a finite range of radii whose values are governed by the temperature of the water and the initial air content in the bubble.

In the analysis for the bubble radii it is assumed that the vapor pressure remains constant during the growth and hence the bubble growth is isothermal. This assumption is a reasonable one near the equilibrium point since the rate of growth is very slow initially compared to the subsequent behavior of the bubble. In addition the assumption is made that the diffusion of air across the bubble boundary is negligible which means that the air content in the bubble remains constant. With these assumptions it turns out that the range of radii for which the equilibrium is unstable is given by

$$\frac{4\sigma}{3(P_V - P_\infty)} \leq R_0 \leq \frac{2\gamma}{P_V - P_\infty}$$

$$\frac{\delta P}{2} \geq P_{A0} \geq 0$$

where P_{A0} is the initial pressure of any air in a bubble of radius R_0, P_V is the vapor pressure of the water at the appropriate liquid temperature, P_∞ is the atmospheric pressure, γ is the surface tension constant for water, and $\delta P = P_V - P_\infty$.

One phase in the cavitation research program of the Naval Ordnance Test Station is to obtain the rate of growth of vapor bubbles near the equilibrium radius by high-speed photography in order to check the theoretical calculations on the equilibrium radii. Such photographs have been obtained in the Cal. Tech. Rept. No. 21-10, but these photographs do not give sufficient detail near the equilibrium sizes of the bubbles. Hence greater magnification of the photographs are needed to record the initial growth of the bubbles.

AUTHOR'S CLOSURE

The discussers' diversified backgrounds and experience in the fields of Physics and Naval Architecture testify in a most striking manner to the interest in the field of cavitation and reflect the need in this field for constant interchange of ideas and information among those concerned with basic mechanisms and theory and those whose ultimate responsibility is the interpretation and application of such information for design purposes. If the point of view taken in TMB Report 712 and this supplement and the scheme adopted in the "unification" of the material has helped in even a small way to establish more clearly the bases for such communication, then it is felt that the results have more than compensated the effort.

Before considering the printed discussions, a word of apology to those who presented discussions appears to be in order. The very short time available for preparation of the discussions and the availability of only a preliminary edition placed an extra burden on the discussers and was occasioned by the writer's failure to have the final edition ready in time for examination before the presentation to the Chesapeake Section of the Society of Naval Architects and Marine Engineers on October 18, 1952. The writer is very grateful not only to those who somehow found the time to send written discussions but also to those who presented oral remarks at the meeting. Thus, it should be observed that the printed discussions refer to the preliminary version of the paper and not to the final report.

Dr. Todd's resumé of the cavitation problems facing the Naval Architect is a particularly valuable adjunct to the present paper because is brings out the necessity for more intimate knowledge of the cavitation mechanism and for extension of theoretical results. With regard to the specific question concerning scaling of propeller experiments, the present methods used at the Taylor Model Basin are not incompatible with the concepts outlined in this paper and with the conclusions reached by Kermeen and Parkin. Furthermore, the present TMB practice of using larger models and higher speeds than were formerly used is designed to reach as large values of the Reynolds number as possible with the facilities available. This is particularly important in attempting to obtain flow similarity and, thus, similarity in the pressure distributions. However, the propeller problem is considerably complicated by the differences between the sea water in which the prototype operates and the fresh water in the cavitation tunnel. Crump concluded that to obtain the same critical cavitation numbers in the TMB water tunnels as in his

experiments with sea water, the fresh water must be supersaturated with air (about 30% supersaturation). This is again compatible with the expected behavior of nuclei in that sea water which is saturated near the surface also certainly contains a large number (relatively) of gas bubbles whether entrained or stabilized on particles (plankton, etc.). Thus, supersaturated fresh water insures the growth of undissolved nuclei to provide the foci for cavitation inception. However, it is not likely that the question will be completely settled until the behavior and role of such nuclei can be studied in detail in the sense suggested in the text of the paper.

With regard to Dr. Todd's question concerning the damage problem, the writer's views are essentially the same as were expressed in the earlier paper (TMB Report 712), i.e., that damage is associated with the pressures arising upon collapse of transient cavities whether resulting directly in fatigue failures or in producing deformations in the crystal structure of a metal which result in internal corrosive electric currents. The absolute pressures which can be developed in the liquid depend upon the permanent gas in the bubble, the rate of condensation of vapor, the viscosity of the liquid, the surface tension, compressibility, etc. and no computations have as yet been carried out including all these variables which enable firm statements to be made concerning the absolute pressures in real liquids. It will be noted that except for surface tension all these variables tend to retard the rate of collapse. Furthermore, accurate measurements of the peak values have not as yet been made since these occur in only the last few microseconds of the collapse cycle and such resolution with presently available instrumentation is difficult to attain. The problem is further complicated by the fact that bubbles near a surface or in pressure gradients do not collapse spherically and the pressures in such cases are less than for spherical collapse. Nevertheless, that the pressures developed are sufficient to cause damage has been convincingly demonstrated by direct experiments as well as indirectly by such experiments as those in which air is introduced into the cavitated region to "cushion" the collapse. Whether the hydrodynamic pressures are <u>always</u> large enough to cause damage is still an open question, however. In TMB Report 712, the writer suggested that another possible mechanism may be associated with impacts produced by jets which might arise as a result of non-spherical collapse of the bubbles, it being known that the impact of drops and jets cause the same type of damage as that observed in cavitation damage.

Attempts have been made to reconcile the possibility of pressures too small for fatigue damage with the observations that damage does occur. Among these are the suggestions of Petracchi* (mentioned above) of corrosive electric currents associated with crystal deformation and of Kyropoulos** of initial damage to the intercrystalline cement which may fail at comparatively small pressures. For non-crystalline substances such as lead, however, the hydrodynamic pressures (or jets) are evidently the direct cause of damage. Additional complications arise when cavitation takes place in a corrosive medium, but, in this case, damage is probably accelerated since the action of the corrosive material and of the cavitation are mutually assisting. Cavitation in this case acts to assist in exposing corrosive material to the surface while the corrosive material weakens the surface and thus lowers the pressures required for cavitation damage. This action is, in a sense, self-propagating in that as damage proceeds additional surface is exposed to corrosive attack and the roughened surface assists in cavitation formation and gives rise to crevices where stress concentrations can occur.

The remarks by Dr. Lerbs are particularly pertinent to the question of propeller scaling discussed above, and he is eminently more qualified by background and experience with the problem of marine propellers to discuss such problems than is the writer. The failure to obtain correlations in Dr. Lerbs' tests of the type observed by Kermeen is perhaps not too surprising since the propeller is a so much more complicated hydrodynamic system than the body of revolution, and detailed control of experiments is a most formidable task. Furthermore, the smallest propeller size that might be tested at Reynolds numbers large enough to insure flow similarity may already be too large for effects of the type found by Kermeen to be observed. In Kermeen's report, it was pointed out that the Reynolds numbers were large enough so that the form of the pressure distribution under non-cavitating conditions, at least, did not change with changing Reynolds number. This is a difficult condition to attain with model propellers.

Among many other complications might be mentioned the problem of the blade shapes used in such experiments. For blades with sharp leading edges, it seems likely that effects

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*Petracchi, G., "Investigation of Cavitation Corrosion", The Engineering Digest, Vol. X, No. 9, Sept. 1949, pp. 314-316

**Kyropoulos, S., "Cavitation Pressures and Damage", Zeit. fur Angewandte Math. und Phys., Vol. II, 1951, pp. 406-410.

such as those discussed above may not be observed even with the smallest model. The reason for this suggestion is that local separation can easily occur at such a leading edge and cavitation can then start easily, the separated fluid moving with the blade and allowing sufficient time for inception to occur. It would be of much interest to perform a comparable set of experiments with propellers having well rounded leading edges so that the minimum pressures occur on the blade surface aft of the leading edge.

With regard to Dr. Lerb's comments on the forces developed on cavitating flows with lift, it might be pointed out that exact theoretical results for two-dimensional flows about polygonal obstacles are available (see, e.g. reference 30 of the paper). However, these computations only include the formulation of the solutions, and derivations of forces have not as yet been performed. The work of Tulin on lifting surfaces will be of particular value in answering the question posed by Dr. Lerbs at least for thin sections. For thick sections, i.e., sections on which the cavity starts along the profile, it will be necessary to resort to exact theory and include the effects of surface curvature. In principle, this is evidently now possible, but the computations will be extremely involved. The high-speed computing machines will be valuable tools in this problem.

The results of Betz and Walchner merit additional comment. Betz approximation theory gives for the lift coefficient $C_L = \frac{\pi}{2}\alpha + \sigma$ where α is the angle of attack. The first term on the right represents the lift coefficient of a flat plate at small angles for Kirchhoff flow. In Walchner's experiments the separation points were fixed (sharp leading and trailing edges on thin sections) so that Betz' assumption that the pressure over the suction side is essentially fixed is a reasonable one. However, the assumption that the pressure distribution on the pressure side is constant seems to have no foundation. Reichardt's assumption (see reference 3) that the non-dimensional distribution remains the same is much more reasonable. Furthermore, Betz' formula which is essentially a flat plate approximation leads to the anomalous result that lift is developed at zero angle of attack. Thus, the good agreement with Walchner's results at very small angles of attack is rather surprising. These remarks are made only to point out the need for rational theory and additional experiments and, thus, to endorse Dr. Lerbs' comments on this problem.

The question posed by Mr. Mandel appears already to have been answered in his observation that separation occurs earlier on the model than on the full scale rudder. If the different

behaviors were due to differences between the properties of sea water and the water in the model basins, just the opposite effects might have been expected since, even for similar pressure distributions, sea water with its high air content would cavitate sooner than the model. Thus, it appears that rudder breakdown is associated primarily with separation (which on such thin lifting surfaces is delayed as the Reynolds number is increased) rather than with cavitation itself.

The writer is very grateful to Dr. Plesset for his very kind remarks regarding the review reports, particularly in view of Dr. Plesset's many personal contributions to the subject and the stimulus he has provided for research in this field among his students and colleagues at the California Institute of Technology.

The preliminary copy of the report available to Dr. Plesset did not contain the observations on hysteresis included in the final copy, and further comments on this phenomenon need not be included here. The question of stability of collapsing bubbles is particularly important in connection with the pressures that will be developed during this phase of the motion, a distorted bubble evidently producing lower pressures than a spherical bubble.

At least two mechanisms, in addition to the wall effects mentioned previously, can be distinguished as leading to deformations of an initially spherical bubble. One is "Taylor instability", i.e., the instability of an interface between light and heavy fluids accelerated one toward the other*, and the other is the deformation produced by the pressure gradients in the field in which the bubble is moving. The question of stability in the above sense has been investigated by Binnie** for spherical surfaces as an extension of Taylor's plane case. He concludes that if surface tension is neglected, the interface is stable if the acceleration is directed toward the lighter fluid and unstable if the acceleration is directed toward the denser fluid. For the Rayleigh empty cavity, this implies that the motion is always stable. However, in an actual case in which the gas phase is compressed to high pressures, both the collapse in the very last stages and the early stages of the subsequent growth would be unstable. Binnie also found that surface tension has a strong stabilizing effect.

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*Taylor, Sir Geoffrey, "The Instability of Liquid Surfaces when Accelerated in a Direction Perpendicular to their Planes", Proc. Roy. Soc. Lond., Ser. A, Vol. 201, 23 May 1950, pp. 192-196.

**Binnie, A. M., "The Stability of the Surface of a Cavitation Bubble", Proc. Camb. Phil. Soc., Vol. 49, pt. 1, Jan 1953, pp. 151-155.

The stability conditions of flows under large tensions mentioned by Dr. Plesset is evidently of the same type as that discussed by Dr. Dergarabedian for superheated liquids. In either case, inception occurs "explosively" in the sense that vaporization of large quantities of liquid takes place very rapidly after the start of cavitation or boiling. Thus, in the venturi nozzle of Crump's experiments, the cavitated region formed after inception under conditions of tension grew immediately and filled the nozzle to an extent corresponding to the size associated with initial inception at vapor pressure and subsequent growth until the same volume flow conditions had been reached. In the case of superheated liquids, the sudden onset of boiling at large amounts of superheat has been known to shatter glass containers.

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